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XI. *Observations on Respiration.**By the Rev. Joseph Priestley, LL.D. F. R. S.*

Read February 25, 1790.

WHEN I wrote the *Observations on the Subject of Respiration*, published in the *Philosophical Transactions*, Vol. LXVI. p. 226. I supposed, that in this animal process there was simply an emission of phlogiston from the lungs. But the result of my late experiments on the mutual transmission of dephlogisticated air and of inflammable and nitrous air, through a moist bladder interposed between them, and likewise the opinions and observations of others, soon convinced me, that, besides the emission of phlogiston from the blood, dephlogisticated air, or the acidifying principle of it, is at the same time received into the blood. Still, however, there remained a doubt how much of the dephlogisticated air which we inhale enters the blood, because part of it is employed in forming the *fixed* air, which is the produce of respiration, by its uniting with the phlogiston discharged from the blood: for such I take it for granted is the origin of that fixed air, since it is formed by the combination of the same principles in other, but exactly similar, circumstances.

Dr. GOODWYN's very ingenious observations prove, that dephlogisticated air is *consumed*, as he properly terms it, in respiration; but, for any thing that he has noted, it may be wholly employed

employed in forming the fixed air above mentioned. He has proved, indeed, that the application of dephlogisticated air to the outside of a vein will change the colour of the blood contained in it. But this might have been effected, as I first supposed, by the simple discharge of phlogiston from the blood, when it had an opportunity of uniting with the dephlogisticated air thus presented to it. He does not, however, seem to suppose, that there is any phlogiston discharged from the blood in the act of respiration, but only that dephlogisticated air enters into it. But that my former supposition, as well as *his*, is true, will appear, I presume, from the experiments which I shall presently recite.

As, in order to determine what proportion of the dephlogisticated air destroyed by respiration is employed in forming the fixed air which is the produce of it, it was necessary to ascertain as exactly as possible the proportion of dephlogisticated air and of phlogiston in the composition of fixed air, I repeated with particular care experiments similar to those which I had formerly made for that purpose.

I heated charcoal of copper in 41 ounce measures of dephlogisticated air of the standard of 0.33, till it was reduced by washing in water to 8 oz. m. of the standard of 1.33. Again, I heated charcoal of copper in 40.5 oz. m. of dephlogisticated air of the standard of 0.34 till it was reduced to 6 oz. m. of the standard of 1.76. And in each of these cases there was a loss of 6 gr. of the charcoal of copper; so that there cannot be more than 6 gr. of phlogiston in 33 oz. m. of fixed air, and consequently that only a very little more than one-fourth of the weight of fixed air is phlogiston.

I heated perfectly well burned charcoal of wood in 60 oz. m. of common air, and found one-fifth of the remainder to

be fixed air, and the residuum of the standard of 1.7. Lastly, I heated  $8\frac{1}{4}$  gr. of perfect charcoal in 70 oz. m. of dephlogistified air, of the standard of 0.46, when it still continued 70 oz. m.; but after washing in water it was reduced to 40 oz. m. of the standard of 0.6, and the charcoal then weighed  $1\frac{1}{4}$  gr.; so that from this experiment with common charcoal, as well as from the preceding with charcoal of copper, it appears, that about one-fourth of the weight of fixed air is phlogiston, and consequently that the other three-fourths are dephlogistified air.

Having done this, I proceeded to ascertain how much fixed air was actually formed by breathing a given quantity both of atmospherical and of dephlogistified air, in order to determine whether any part of it remained to enter the blood, after forming this fixed air.

For this purpose I breathed in 100 oz. m. of atmospherical air, of the standard of 1.02, till it was reduced to 71 oz. m. and by washing in water to 65 oz. m. of the standard of 1.45. When the computations are properly made, as directed in a former Paper, it will appear, that, before the process, this air contained 67.4 oz. m. of phlogistified air, and 32.6 oz. m. of dephlogistified air; that after the process there remained 53.105 oz. m. of phlogistified air, and 11.895 oz. m. of dephlogistified air; and that there were only 6 oz. m. of fixed air produced; for the quantity absorbed during the process could only have been very inconsiderable. It will therefore be evident, that, in this experiment, 20.7 oz. m. of dephlogistified air, which would weigh 12.42 gr. disappeared; whereas all the fixed air that was found would only have weighed 4.4 gr., and one-fourth of this being phlogiston, the dephlogistified air that entered into it would have weighed only 3.3 gr.; consequently

9.12 gr. of it must have entered the blood, which is three times as much as that which did not enter, but was employed in forming the fixed air in the lungs.

I breathed in 100 oz. m. of dephlogisticated air, of the standard of 1.0, till it was reduced to 58 oz. m., and by washing in water to 52 oz. m. of the standard of 1.75, with two equal quantities of nitrous air. The computations being made as before, it will appear, that, before this process, this air contained 66 oz. m. of phlogisticated, and 34 oz. m. of dephlogisticated air; and that after the process there were 30.368 oz. m. of phlogisticated air, and 21.632 oz. m. of dephlogisticated air. In this case, therefore, the dephlogisticated air that disappeared was 13.3 oz. m. weighing 7.8 gr. and the fixed air was 6 oz. m. weighing 4.4 gr.; so that here also about three times as much entered the blood as did not.

These experiments I repeated many times, and though not with the same, yet always with similar, results, the greatest part of the dephlogisticated air, but never the whole, passing the membrane of the lungs, and entering the blood.

When the results above mentioned are compared, it will appear, though the observation escaped Dr. GOODWYN, that part of the phlogisticated air entered the blood, as well as the dephlogisticated air; or, which is the same thing, that the dephlogisticated air which was consumed was not of the purest kind. This experiment I repeated so often, and always with the same result, that I am confident I cannot be mistaken in this conclusion. This fact, of which I had no previous expectation, I first thought might be accounted for by supposing, that the two constituent parts of atmospherical air, *viz.* the phlogisticated and dephlogisticated air, are not so *loosely mixed* as has been imagined; but rather that they have some principle

of *union*, so that, though they may be completely separated by some chemical processes, they are not entirely so in this; but that the dephlogisticated air, passing the membrane of the lungs, carries along with it some part of the phlogisticated with which it was previously combined. But, at the obliging suggestion of Dr. BLAGDEN, I now think it more probable, that the deficiency of phlogisticated air was owing to the greater proportion of it in the lungs *after* the process than *before*.

There will always be some uncertainty in the results of the long-continued respiration of any kind of air, as at the last the operation becomes laborious, and the quantity inspired and expired is therefore much greater than at the first. But I was aware of this circumstance, and endeavoured to obviate the effects of it, by leaving off with my lungs as nearly as I could judge in the same state of distention as when I began, which was always after a moderate expiration; so that two or three ounce measures would have made a very sensible difference, as any person will find by actual trial.

